

Review for “Mixed-Phase Microphysical Evolution in Large Eddy Simulations of Tropical Cumulus Congestus: Developing and Evaluating a Laboratory-based Ice Multiplication Parameterization of Freezing Drops” by Stanford et al

Overview

This manuscript presents a novel parameterisation for fragmentation of freezing drops (FFD). Using LES simulations they simulate a tropical convective case, showing the impacts of the new parameterisation in both bulk and bin microphysics schemes. The results show that the bin and bulk schemes produce clouds evolving differently, but both simulations show negligible impacts from the inclusion of FFD. The authors hypothesise this is due to the uncertain nature of the amount of splinters produced during each event or due to the low immersion freezing rates. A perturbation of $\times 10$ to the splinter generation for the FFD parameterisation is required to produce results that show substantial changes to the clouds.

The paper is technically written with a large focus on the implementation of the microphysics. Of particular interest to me is the difference in collision rates between the bin and bulk schemes, offering a potential explanation for how the additional splinters impact the PSD in bin and bulk, respectively.

While it offers some interesting implementations and scientific outcomes, I do not think it is enough for publication in its current form. These are my main concerns;

- The detailed look at only 1 case study reduces any possibilities of extending the outcomes to even similar cloud systems
- While the new parameterisation is interesting, the lack of comparison to other FFD parameterisations makes it impossible to extract information to whether this new parameterisation is in fact a useful contribution or whether it produces a similar response to established parameterisations
- If the immersion freezing rates are one of the bottle necks in the simulation, why is this not further explored through perturbations?
- The number of splinters generated from each event is based on two studies from the 70s, while I can appreciate the work done to come up with a function for the generated splinters, this is a weak link and uncertainties in this expression should be better discussed
- The single perturbation by $\times 10$ feels arbitrary and I am missing any justification why this factor is used, and only this factor is used, if the purpose is to systematically study the impact of the parameterisation, several perturbations should be included
- The lack of N_{ice} seems quite serious as the main point of the paper is to evaluate an N_{ice} generating mechanism. The “infer the contribution of FFD based on PSD structure” has to be better described and justified

I would suggest major revisions and possible reconsideration upon the inclusion of:

- Implementation of other established FFD parameterisations such as Phillips et al 2018 for comparison
- A systematic perturbation strategy based on the uncertainty in immersion freezing and splinter generation

and ideally also:

- one additional case study

Minor Comments

The authors introduce FFD as a process but refers to it through the immersion freezing scheme as ABIFM-SIP. I would prefer a consistent use of the word FFD throughout the manuscript when referring to this process. Similarly, the simulations should be named BULK_FFD etc.

Furthermore, in regard to the use of the words “collisional SIP”. To remove any confusion and possible misunderstandings, as this is not what is commonly referred to as ice-ice collision, I would prefer the use of “collisional FFD”.

L10-12 Has this been properly shown or does it refer to the hypothesis given in 4.3?

L96-97 This sentence is unclear to me. Is the $\times 10$ perturbation based on this assumption?

L100 One may pose the question to why the laboratory setup did not include methods already established in the 70s to be able to estimate the number of splinters generated, I appreciate that this is not part of the current manuscript but a justification for this is required as it severely limits the parameterisation development

L226 Why is the organic fraction set to 0.3? Is this perhaps a parameter that should be taken into account when doing perturbations to the immersion freezing?

L227 How can the PRE be parameterised for both immersion freezing and collisions? Are both of these processes measured in the lab, or what are the assumptions included here?

L269 According to L232 the splinters have a $D \sim 2.25 \mu\text{m}$, here it says mass equivalent size of $5 \mu\text{m}$? Do the bin and the bulk scheme implementations differ in the assumed size of the generated splinters? How does this impact the results?

L257-272 Could the authors compare this to mode 1 and mode 2 of Phillips et al 2018 parameterisation?

FIG6 A white background would show a,d better in a print out. Furthermore, the horizontal temperature lines, (constant with time?), should be marked in the plot or a legend which temperature they refer to

L309 Could the authors just explain to me the benefit of a mass-weighted time series? Does it improve the comparison between BIN and BULK? Otherwise, to me it simply makes the comparison between the plots more challenging to interpret as I would say that the absolute increase in N_{ice} is more interesting than the mass-weighted?
Also, the units should then be adjusted accordingly?

L359 s_{INP} (L649) or S_{INP} (here)?

L370 and further, just something I am curious about; could the inclusion of FFD increase the supersaturation through an increase in latent heating and buoyancy such as seen in Waman et al 2026, such that more cloud droplets are activated in these simulations? Have the CCN activation rates been evaluated?

L430 Can the authors quantify the bias stated between simulations and observations?

FIG12 Lines are impossible to see in print-out. The addition of dashed lines would help the readability.

Data availability; Please make processed simulation files available on a public repository.

Technical Points

L34 Perhaps the word “hypothesised” fits better than “well-established”. As the authors mention in L39, one of these processes have been recently found to not have an appreciable impact

L63 Missing a word perhaps?

L67 “specific” instead of “narrow”?

L140 an additional “in”

References

Phillips, V. T. J., S. Patade, J. Gutierrez, and A. Bansemer, 2018: Secondary Ice Production by Fragmentation of Freezing Drops: Formulation and Theory. *J. Atmos. Sci.*, **75**, 3031–3070, <https://doi.org/10.1175/JAS-D-17-0190.1>.

Waman, D., Meusel, J., Keshtgar, B., Wallentin, G., Barthlott, C., Patade, S., Shete, S., Prabhakaran, T., Fievet, R., Finney, D., Blyth, A., and Hoose, C.: Impacts of secondary ice production on the microphysics and dynamics of deep convective clouds in different environments, *Atmos. Chem. Phys.*, **26**, 11371–11407, <https://doi.org/10.5194/acp-26-11371-2026>, 2026.